Universität St.Gallen

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March 2020 Discussion Paper no. 2020-06

School of Economics and Political Science,
Department of Economics

University of St.Gallen

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Publisher: School of Economics and Political Science
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Electronic Publication: http://www.seps.unisg.ch

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[1] We would like to thank participants in the seminars at CERNA, the RFF-CMCC European Institute for Economics and the Environment, the Ratio Institute, and Universitat de Barcelona for their valuable comments. We gratefully acknowledge funding from FORMAS, the Swedish Government's Research Council for Sustainable Development, research grant nr. 2017-00255. Caterina gratefully thanks CERNA Mines ParisTech for hosting her research visit.

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Abstract:

This paper evaluates the spillover effect of a pro-environmental policy that introduced organic-waste sorting bins on a non-targeted behavior: total household waste. Using an administrative dataset on household waste from Sweden, we find large reductions in waste due to (i) information about the benefits of organic waste recycling and (ii) the provision of organic-waste bins. Our empirical strategy utilises spatial random variation in the administrative implementation of the reform. Our findings are compatible with attention spillovers in a framework with limited attention.

Keywords

Behavioral spillovers, environmental policy, limited attention, household waste, staggered difference-in-difference

JEL Classification

D01, D04, D12, D83, D9, H41, O31, Q5

1 Introduction

Waste reduction is a critical target for policy makers. Global municipal solid waste reaches two billion tonnes annually, which corresponds to 0.74 kg per person daily, on average. At least one third of it is disposed of and managed in non-sustainable ways: 19% is recycled or composted, 11% is incinerated, and the rest is disposed of in landfills (37%) or through open dumping (33%). In 2016, solid waste caused five percent of total carbon dioxide equivalent greenhouse gas emissions, excluding the impact of its transportation. Fifty percent was due to organic waste alone. Projections suggest that global solid waste will reach about 3.4 billion tons by 2050 if the current sector conditions do not improve (Kaza et al., 2018).

Local governments are increasingly looking for efficient ways to nudge consumers into environmentally sustainable waste production behaviors. Pro-environmental interventions typically target specific activities such as reusing, recycling or reducing waste. However, recent research in the context of conservation efforts suggests that increased engagement in a specifically targeted pro-environmental activity may crowd out other contributory behaviors (Dorner, 2019; Tiefenbeck et al., 2013). The environmental psychology literature suggests that negative behavioral spillovers could originate from the tightening of time and budget constraints, as well as from a *behavioral rebound* or a *moral license* to relax the effort that agents dedicate to non-targeted behaviors.[1] Negative behavioral spillovers may thus lead to an inferior final net equilibrium (Ek, 2018). On the other hand, increased attention for a pro-social or self-transcending pro-environmental goal may encourage effort across many activities that are not directly targeted (Evans et al., 2013).[2]

Understanding policy spillover effects across behaviors is particularly important in an environmental context, where agents' contributions to a common social goal can be achieved through multiple activities. Furthermore, waste is particularly suitable for analyzing behavioral spillover effects in a public good context because, as a byproduct of a repetitive activity (consumption),

[1]See Nilsson et al. (2017), Dolan and Galizzi (2015), and Truelove et al. (2014) for a review of the literature on behavioral spillovers in the field of environmental psychology.

[2]Similarly, recent research on donations has found that incentivizing aid to one beneficiary does not crowd out the total amount that users of an online platform give to the other registered charities (Meer, 2017).

it is characterized by low salience. The existing literature on waste conservation policies focuses on the effects of unit pricing schemes on the reduction and recycling of the targeted waste type (Bueno and Valente, 2019; Bucciol et al., 2015; Allers and Hoeben, 2010; Reichenbach, 2008; Usui, 2008; Jenkins et al., 2003). However, the behavioral literature suggests that extrinsic financial incentives may crowd out intrinsic motivations (Gneezy and Rustichini, 2000; Benabou and Tirole, 2003). In the context of waste generation, this would mean that households may feel entitled to decrease avoidance efforts by paying a price for the waste they generate. In addition, among the downsides of pay-as-you-throw schemes there are instances of illegal dumping (see, e.g., Bucciol et al. 2015). Campaigns based on non-pecuniary incentives and information, therefore, still occupy a crucial position in the set of available instruments that policy makers can use to incentivize solid waste reduction and recycling. Besides a recent contribution concerning recycling volumes based on aggregate municipal-level data (Ek and Miliute-Plepiene, 2018), we are not aware of further rigorous causal evidence of the effects of non-monetary incentives for conservation efforts on behavioral spillovers in the economics literature.

Our paper contributes to filling this gap and estimates the effects of a policy that targets enrollment in curbside separation and collection of organic waste. We evaluate the take up of the organic-waste bins as well as the impact of the policy on volumes of household waste, which are not directly targeted. Such an effect represents a behavioral spillover from a pro-environmental policy on a non-targeted but related environmental effort. More precisely, we evaluate an intervention with two components: (i) the provision of information regarding the benefits of organic waste reusage (for example as biogas for local transportation) and (ii) the introduction of elective curbside bins to separate organic from residual waste. The policy maintained existing unit prices (identical for all waste types) and made the organic-waste and residual bin combination about 90 USD/year cheaper than the existing default unsorted bins, for those who enrolled.[3]

We use novel data on waste generated by households in the Swedish municipality of Partille. Our dataset has several unique features. First, we observe the exact amount of waste produced

[3] Packaging waste could be disposed of at recycling stations free of charge, both before and after the policy was introduced.

each month by each household from 2012 to 2017. Our dataset is administrative and measurements are generated by a digitally equipped system, with an electronic scanning device attached to the collection arm of the waste trucks. This device scans the specific barcode from each single bin. To the best of our knowledge, our paper is the first to use administrative waste data at household level to address the direct and non-targeted behavioral effects of a pro-environmental intervention on household waste production. Aggregated data does not allow for the analysis of treatment effect heterogeneity. In addition, units of aggregation (such as municipalities) are often different in terms of infrastructure and socioeconomic composition, which hampers the econometric identification of policy effects. Self-assessment survey data is well known to be prone to measurement error due to Hawthorne effects. Furthermore, repetitive activities such as waste generation are characterized by decreased salience, which implies that self-assessed waste data is likely to be imprecise. Our dataset does not suffer from behavioral bias or recollection-noise variance.

Second, a crucial advantage of our dataset is that we observe the precise timing of information dissemination. In particular, we observe (i) the date each household was informed about the content and purpose of the future reform and (ii) the date each household was asked whether it wanted to participate or remain in the old system with collection of only unsorted waste. Accounting for information dissemination is important as it allows us to capture possible anticipation and adjustment effects. This in turn allows us to precisely measure the marginal effect of the policy implementation, as well as the total effect of information and implementation. To our knowledge, this is the first paper in the context of behavioral spillover effects due to an environmental policy intervention that explicitly incorporates anticipation effects. Existing identification approaches rely on difference-in-differences (or its fixed-effects panel counterpart) and do not account for pre-policy information effects, as in Ek and Miliute-Plepiene (2018).

Our identification strategy utilizes a natural experiment triggered by the staggered introduction of curbside organic waste separation in the municipality. In particular, the implementation started at different times in four different sub-regions of the municipality. The division of the municipality into four sub-regions and the timing of the implementation responded only to waste management purposes and did not overlap with further administrative partitioning or policy changes. Thus, the staggered implementation provides a source of exogenous variation, in combination with household and time-specific fixed effects. To identify the treatment effect of the policy, we compare treated with not-yet-treated outcomes in a staggered difference-in-difference framework, a strategy that is similar to the evaluation of phased-in controlled experiments, see, e.g., Duflo et al. (2007). This setting allows us to estimate the impact among targeted and complying households and to distinguish between the effect of information and the effect of enrollment in the sorting scheme.

We find that 70% of households adopted the curbside organic waste disposal technology and that 18% composted privately. To further determine the impact of the policy on household waste production, we analyze the weight of overall domestic waste, which consists of a combination of organic and residual (or unsorted) waste, net of recycled items. Both dissemination of information and the actual implementation of the new waste system significantly reduced the amount of waste generated by households. Our estimates indicate that the total amount of monthly waste reduction was 2.5 kg, or 8% of the average 31 kg/month per household, implying that the impact of the policy was economically significant. Furthermore, an analysis of the heterogeneity of the effect indicates that already efficient households had less room to adjust their behavior and, thus, the highest impact of the policy was noted for the less efficient households. Finally, we also analyze the persistence of the effect after the delivery and recording of the bins. We find that it is significant in the first eight months, which is consistent with attention being the main driver of the behavioral spillovers. In particular, in the context of a behavioral spillover, our results suggest that, as waste separation and waste generation are closely related activities, increased attention to separation (triggered by the policy) likely spanned to waste generation behavior. This "correlated attention shock" is straightforward to incorporate in existing behavioral microeconomic models. Building on the literature on limited attention (Gabaix, 2014), we interpret our findings through a framework of attention-driven contributions in a multi-activity setting. We show that our results are consistent with a setting in which agents gain utility by adhering to the salient goal of behaving pro-environmentally and by pursuing it consistently across different but related activities. When one activity receives a boost in attention, activities that are closely related "profit" from that boost.

We contribute to the literature on economic policy and behavioral spillovers in terms of both findings and methodology. The interest in behavioral spillovers originates in the psychology literature (see e.g. Nilsson et al., 2017 and Truelove et al., 2014 for a review). Its major focus has been on negative spillovers due to moral license. Positive spillovers are generally discussed in the context of consistency theory and "cognitive dissonance" (Rabin, 1994), with identity theory being a sub-branch. According to the latter theory, an individual that engages in an first pro-environmental behavior consequently keeps behaving pro-environmentally, in an effort by the to maintain a consistent self-image.[4] While there is also abundant literature on the psychology of limited attention (Gabaix, 2019), we are not aware of any other paper that shows positive behavioral spillovers that originate from a shock in salience spanning to correlated activities.

On the methodological side, the overwhelming majority of studies on behavioral spillovers have used lab experiments or field experiments and self-reported outcomes at best (Nilsson et al., 2017; Truelove et al., 2014). Our paper is among the very few studies evaluating the behavioral spillover effects of an actual, real-world, policy intervention, with a causal interpretation. The study that is closest to ours is the one by Ek and Miliute-Plepiene (2018), who analyze the effect of the introduction of organic waste separation on recycled waste in Sweden using aggregate municipality-level data. They estimate that organic waste separation increases the overall weight of packaging items disposed at dedicated municipal recycling stations. Their data does not allow them to study the response to the policy implementation in terms of organic and residual household waste. Thus, our analysis provides novel results that crucially complement the existing evidence: we show that, in response to a policy that promotes the recycling of organic waste, households decrease their overall waste, net of that disposed of at recycling stations. Taken together, the two studies suggest that the introduction of organic waste separation generated higher recycling *and* a reduction of non-recycled and non-composted waste, which results in cleaner waste streams and the production of biogas from organic waste within a circular system.

The paper is organized as follows: Section 2 describes the setting, the policy design and implementation, and the data. Section 3 presents the empirical strategy. Section 4 reports the results of the policy evaluation, heterogeneous effects by baseline waste production, and the evolution of the effect over time. Section 5 discusses the economic interpretation of our findings, and Section 6 concludes with policy implications.

[4] An often used special case is the so-called "foot-in-the-door" effect: see Thøgersen and Crompton (2009).

2 Data and background

2.1 Institutional background: municipal waste collection

Swedish local administrations have gradually introduced curbside collection of household organic waste since the 1990s. The context of our analysis is the municipality of Partille, where the waste management department implemented a policy that introduced the separation of organic waste in a staggered way along different collection areas, starting in the summer of 2013. The municipality is situated in the south-west of Sweden, on the outskirt of the city of Gothenburg. It consists mostly of residential detached single-family houses. Household waste is regularly collected door-to-door by a truck that weighs and records the weight of each household's bin. The pricing scheme for waste consists of a fixed and a variable component. Households pay a per-unit price for each kilogram of waste they produce (∼0.20 USD/kg[5]) and a fixed annual fee per bin that depends on capacity - the larger the bin, the higher the fee. There are also several recycling centers, currently 32, where households can bring their plastic, paper, glass, and metal waste free of charge. Prior to the implementation of the reform, organic and residual waste was not collected separately, and hence only the total amount of waste per household was recorded.

2.2 The policy

The policy disseminated information about the benefits of sorting and recycling organic waste by mailing out brochures and introduced the option of using a special organic-waste bin. Importantly, the intervention maintained both the pre-existing pay-per-weight pricing scheme and the possibility of discarding recyclable waste items made of plastic, metal, glass, and paper at recycling centers free of charge. This excludes the interference of additional recycling or unit pricing-based monetary incentives. Figure 1 shows the four areas in which the policy was implemented in a staggered way. The areas were only designed for waste management reasons and their partition had no further overlap with any political or administrative purposes. In July 2013, the municipality sent a brochure to all households to inform them about the option of having an additional bin dedicated to organic

[5]Corresponding to the fee of 1.84 SEK/kg at the current exchange rate.

waste at no extra cost, and the future roll-out of the implementation. The brochure presented also an average composition of household waste by type (26% recyclable packaging items, 29% organic, and 45% residual) and illustrated the benefits and pro-environmental consequences of organic waste recycling, such as its transformation into biofuel for use by public transportation buses in a circular system. In a next step (see the timing in Table 1), the municipality sent a second brochure that specified the starting date of the distribution of bins for each area and the costs for each possible bin combinations: (i) an organic-waste bin and a residual bin, (ii) only a residual bin (meaning that the household has a private composting device), and (iii) an unsorted bin, which corresponded to the status quo for all households that were not already composting organic waste on their own. In addition to the three possible waste sorting regimes, households could also choose the size of each bin (190, 240, 370, or 660 liters), and the frequency of collection (weekly, biweekly, monthly, or less often). For a capacity of 190 liters and biweekly collection (the highly prevalent choice), the price for the unsorted bin is the equivalent of 230 USD/year[6], while using the residual bin costs 120 USD/year[7], and there is no fixed annual cost for the organic-waste bin. Households that sign up for a double-bin regime or a compost thus save about 110 USD/year compared with the default unsorted waste scheme.[8] The only novelty concerning costs is, therefore, the introduction of a different annual price for each bin type, favoring organic waste separation. By the scheduled date, households made their choice and communicated it back to the municipality by mail[9] or email. In the absence of an active choice, households would be assigned the unsorted scheme, as the brochure explained. Households that sign up for the double bin option are only allowed to throw organic waste into the organic-waste bins. If collectors find the wrong type of waste in a bin, they will reassign the household to an unsorted regime starting in the following month.[10] Any other type of household waste can be thrown in residual and unsorted bins, including packaging items and

[6]2255 SEK.

[7]1127.50 SEK

[8]For higher capacities, the fixed price ratio between unsorted and residual is always 2:1.

[9]No stamp was required and the return address was already written on it.

[10]Evidence of a change in collection regime in our sample is limited to 0.46% of all post-policy observations. Excluding those observations or dropping the corresponding households from the sample does not affect our estimations. Results are available upon request.

paper. Just as prior to the policy, households can take plastic, glass, paper, metal, and cardboard waste to recycling stations scattered across the municipality. Table 1 reports the precise timing of the policy implementation for each area.[11]

2.3 Data and descriptive statistics

Our data source is the administrative registry of Partilles waste management division. The data reports the weight of each waste bin (in half kilograms) for each collection, from January 2012 to May 2017. It also includes address and area of collection, bin type (unsorted, residual, or organic), size, weight, and date and time of collection, with unique household and bin identifiers. To match each bin with the respective household, we limit the analysis to single-family dwellings. From the initial sample of 5,606 households, we exclude those that do not correspond to single-family dwellings (402 households), those with invalid recordings or duplicate bins (309), and those with missing or invalid bin-combination information at the time of policy change in the corresponding area (96 households).[12] Our final sample includes 4,324 households with more than 270,000 household-month observations. Table 2 reports the descriptive statistics. The average monthly production of household waste over the entire period is 31 kg. Seventy percent of the households adopted the double-bin option when the policy was introduced, while 18 percent opted for private composting and the disposal of only residual waste, and 12 percent remained with the unsorted bin option. The table reports also the average weight before and after the policy for each group, defined according to the choice made at the time of the policy change.[13] "Unsorted" households

[11]The actual recording of organic waste started with some delay after the official policy implementation. Table 1 reports a gap between the officially announced start of collection and the actual recording start. The data shows that, in those months, some households already started disposing organic waste in the specific bin but the municipality did not record it yet (see figure A.1 in Appendix). For this reason, we exclude from our empirical analysis the period in which organic-waste bins were already distributed but not accounted.

[12]We also exclude households whose monthly weight exceeds the mean (31 kg) by more than five standard deviations (218). To avoid mismatching bins to different families that moved in and out of the apartment and to exclude vacation houses, we drop households with 0 kg/month recorded for more than half a year (257).

[13]Three hundred and twenty nine households changed their choice after the initial month, and robustness checks that exclude these households do not change the results.

produced on average 37 kg of waste before the policy change, "double-bin" ones 35 kg, and "residual only" households 20 kg (which suggests they may have already had a composting bin). Figure 2 illustrates the evolution of mean household waste (both organic and residual) in the 20 months before and after the actual collection of organic waste (month 0, thick vertical line). Households are grouped into three types according to their collection choice when the policy was introduced. It is worth noting that the three types report significant differences in mean monthly waste not only after but also before the policy introduction. In the period immediately preceding the recording of organic waste (the thin vertical line, which coincides with the "official start" in Table 1), we observe a large drop in waste for the double-bin group. This is due to the fact that the municipality had already delivered organic-waste bins but only started recording their weight at a later time, as described in Table 1.[14] To tackle this drawback, we drop from our analysis the observations that lie in the interval between the distribution and the recording. Their inclusion would lead to an underestimation of the impact. As a robustness check, we also include them and control for the mis-recording. Figure 3 shows the evolution of household's monthly waste in each area. The back vertical line indicates the dissemination of the first brochure. The grey vertical lines represent the start of organic waste recording for each area. The huge drop in monthly weight for area 1 just before the policy introduction once again represents the fact that bins were already distributed (see "official start" dates in Table 1), but the waste management company did not record their weight prior to February 2014. This effect is amplified by the local polynomial function smoothing, especially for Area 1 (see the Appendix). Two features emerge from the figure. First, households in the four areas have different average amounts of monthly household waste already at the beginning of the period. Second, the four areas report parallel trends prior to the reception of the first brochure (July 2013), which is relevant for our identification strategy and confirmed in a placebo test presented in Section 4.

[14]For households in Area 1, there is a larger gap between the delivery of the bins and the actual start, see Table 1 (and Figure A.1 in the Appendix).

3 Empirical strategy

3.1 Identification

We cast the policy evaluation problem in the Rubin causal framework. Let $D = (D_1, D_2)$ be the individual random treatment vector. D_1 denotes the random enrollment of a household into the curbside waste collection scheme. D_1 can take the value of 0, 1, or 2, corresponding to the regimes "no enrollment" (i.e., the household maintains an unsorted bin), "two bins" (organic and residual), or "only residual bin", respectively.[15] The binary r.v. D_2 denotes the distribution of the second information brochure, with 0 meaning not yet distributed. Note that D2 has the characteristics of an intention-to-treat (ITT) variable, since the household can choose not to open the letter (which is unobserved to us).

We are interested in the effect of D on the amount of household waste generated in a month. A comparison of the averages of individual outcomes Y_{it} (where t denotes month and i the household) in the different groups of D could produce biased estimates because of potential omitted determinants of household waste that vary at the same level of aggregation (such as the availability of grocery shopping facilities as well as socioeconomic characteristics). Households in different areas may have heterogeneous socioeconomic traits that correlate with household waste production. This is particularly likely for D_1, since D_1 is a choice variable and is thus related to hidden environmental preferences and socio-economic characteristics. Denote all such unobserved characteristics by U. Because we have no data that allows us to directly control for those characteristics, our identification strategy utilizes the longitudinal structure of our data and the random order of implementation among the four regions within the municipality. In particular, the policy implementation timing was not related to any relevant household characteristics. Thus, we can consider the points in time of (1) implementation of the policy (distribution of the organic-waste bins), say T_1, and (2) sending out the second brochure, say T_2, as exogenous:

$$(T_1, T_2) \perp\!\!\!\perp U. \tag{1}$$

[15] Alternatively, we could define D_1 to take the aggregated values $0, 1$ corresponding to "no enrollment" (as above) and "enrollment", respectively, the latter being the aggregation of the two cases coded as 1 and 2 above.

With these considerations in mind, our main results are based on two different approaches of estimating the following regression:

$$Y_{i,t} = \beta_0 + \beta_1 D_{1,i,t} + \beta_2 D_{2,i,t} + \alpha_i + \delta_t + \varepsilon_{i,t}, \tag{2}$$

where α_i is a household fixed effect, δ_t denotes month and year fixed effects[16], and $\varepsilon_{i,t}$ is the idiosyncratic noise term. The dependent variable is total household waste in kilograms per month, computed as the sum of all collections per month for each household. With this notation, $U_{i,t} = (\alpha_i, \varepsilon_{i,t})$ and $D_{2,i,t} = \mathbb{1}\{T_{2,i} \leq t\}$. In our first specification, we estimate (2) with a standard fixed-effects (FE) panel estimator. The underlying assumption is that all unobserved determinants of Y related to D_1 are subsumed in the household fixed effects α_i.[17] This assumption is violated if for example environmental preferences are time-varying, or households change their composition (which we do not observe) around the time of the policy change and it affects their decision to enroll in organic waste separation. To account for this possibility, in our second approach and as our second specification, we estimate (2) with an instrumental variable (IV) FE estimator. We instrument the potentially endogenous enrollment choice $D_{i,t}$ with the implementation of the policy in the respective residence area in a two-stage least-squares estimation. In particular, we define $P_{i,t} = \mathbb{1}\{T_{1,i} \leq t\}$ and we instrument $D_{i,t}$ with $P_{i,t}$. $P_{i,t}$ can be interpreted as an ITT variable.

In addition (third specification), we also omit the sole mediator $D_{1,i,t}$ of $P_{i,t}$ and estimate the equation

$$Y_{i,t} = \beta_0 + \beta_2 D_{2,i,t} + \beta_3 P_{i,t} + \alpha_i + \delta_t + \varepsilon_{i,t}. \tag{3}$$

While β_1 in (2) reflects the effect of *enrollment* in the policy reform, the coefficient β_3 represents the *direct effect* of the policy reform. Put differently, β_1 is the treatment effect and β_3 the ITT effect.

[16] Time fixed effects crucially allow us to control for the seasonality of waste production.

[17] Those who choose to enroll in organic waste sorting, either with the bin provided by the municipality or with a private composting device, are a subset of the total sample. We observe that 70% of the households in our sample adopt two bins, 18% opt for private composting of organic waste, and the remaining 12% stick to an unsorted bin.

3.2 Discussion of hidden assumptions

We discuss two implicit assumptions behind our empirical approach. The first one, SUTVA, underlies all of our three specifications:

$$Y_i = Y_i(D). \tag{4}$$

SUTVA is hidden in the notation in equation (4) and postulates that the potential outcome of unit i depends only on the treatment received by that outcome (the so-called "no interference" condition: see, e.g., Rubin, 1986). In principle, we cannot exclude that there are spillover effects, e.g., among neighbors, implying that a reduction in waste is due to neighbors' behavior rather than the policy itself. However, the amount of waste generated by a household is not observed by other households because bins are stored within private properties. Therefore, spillover effects among neighbors are unlikely. Nevertheless, we cannot preclude equilibrium effects, and thus the estimation results should be interpreted with this limitation in mind. Yet, for all practical purposes, the policy makers should be interested in an average effect in the presence of equilibrium effects, since the policy is comprehensive (i.e., everyone receives the treatment).

A second hidden assumption is the exclusion restriction on P_{it} implicitly assumed in equation (2) for our first two specifications. Its interpretation is that the process of implementation of the new bin system does not have any further effect on a household beyond the effect through household participation, conditional on also receiving the second brochure (i.e., conditional on D_2). In principle, it is possible that the implementation had an effect even for those who refused to participate. Those households, however, did not receive any further information after signaling non-participation. Thus, the start of the implementation was not related to any information flow for those households. Again, an effect through informal information transmission (spillovers) cannot be ruled out and we make the reader aware of this pitfall.[18] Our third specification – equation (3) – does not suffer from this potential drawback.

[18]Note that dropping the nonparticipants from the sample and estimating the effect only on the participants would lead to a biased effect because we would then effectively condition on an outcome, and thus on an endogenous variable.

4 Results

4.1 Information, enrollment in curbside organic waste separation, and household waste

Table 3 contains the results of our main three specifications. The first column reports the FE estimates of equation (2). The coefficient for enrollment in organic waste separation is negative and statistically significant at the 1% level. The adoption of organic waste separation is associated with a reduction in household waste by 1.219 kg per month per household. This represents roughly 3.9% of the average monthly household waste and is thus of economically relevant magnitude. The effect of the information brochure sent before the new collection implementation, measured by β_2, is slightly larger in magnitude and amounts to a 1.299 kg reduction in monthly household waste. This corresponds to a 4.2% reduction in the average amount of waste produced by a household each month. Thus, together the enrollment in organic waste separation and the second brochure induce an 8% decrease in produced waste. The second column of Table 3 reports the IV estimates, with the policy implementation timing as an instrument for the potentially endogenous enrollment decision. The estimated coefficient $\hat{\beta}_1$ is negative and significant and equal to -2.121. It is larger in magnitude than the corresponding coefficient in the first specification by almost 74%. The difference reflects the selection bias arising from the endogenous choice to enroll in the new system. The direction of the bias suggests that more efficient households' self-selection into organic waste separation biases the results downwards in the FE regression. In other words, households that are more likely to enroll in organic waste separation may also be more efficient in producing lower amounts of waste for end treatment, and thus have a lower capacity for waste reduction. The coefficient of the second brochure is also negative and significant (equal to -0.829), and slightly smaller than its counterpart in the first column. Similar to the first specification, the IV shows that enrollment into the policy and the second brochure together induce a 9.5% decrease in produced waste. Column 3 contains the first stage estimates. The F-statistic is very high, which indicates that the instrument is strong. The coefficient of the instrument is equal to 0.894 and is significant at the 1% level. Thus, the policy reform had a very high uptake. Finally, column 4 of Table 3 contains the estimates of specification (3). The estimated coefficient β_3 is equal to -1.896 and is significant at the 1% level. Consistent with the evidence presented so far, the policy implementation reduced the total amount of waste.[19]

Next, we perform several robustness checks. First, we exclude all households who opted for private composting. Those households can be identified in the data from their decision to have only a residual bin. The rationale for this check is that organic waste produced by these households after the treatment cannot be measured and, if some households did not have a composting device prior to the reform, this will mechanically drive the effect upward. This change will also lead to a drastic shift (downward) of their ranks in the waste distribution. Table 4 presents the results for the restricted sample. The estimates for both treatment variables are negative and significant and very similar to the corresponding estimates from the main sample. The relevant coefficients are all smaller in magnitude except the IV coefficient for enrollment. The IV estimate of β_1 is now -2.188 (while it is -2.121 in the full sample), which suggests that selection into organic waste separation with a bin provided by the municipality is even higher within the sub-sample of households that do not compost privately. Interestingly, the coefficient of the brochure (β_2) is now smaller, suggesting that privately composting households may also react more strongly to the provision of information. However, the results from Table 4 must be interpreted with caution since we condition on an outcome (and thus likely endogenous) variable.

Second, we challenge assumption 1. In particular, we implement a placebo test in the spirit of a parallel trends test, typical of the difference-in-differences literature. We replicate the estimation of the policy impact in the same way as in equations 2 and 3, but taking lagged values of T1 such that the same schedule can be precisely replicated within the months preceding the mailing of the first brochure (July 2013). For example, placebo P corresponds to February 2012 for Area 1, and the other areas follow according to the timing described in Table 1. The results of the fixed effects and IV estimations are described in Table 5. Note that we exclude all observations after July 2013 in order to get unconfounded estimates. Reassuringly, we find that none of the estimation procedures produce a statistically significant impact of the policy in the lagged time period.

[19] We note that the estimated direct effect of the policy β_3 (-1.896) is exactly equal to the product of the IV-estimate of β_1 (-2.121) and the first-stage coefficient of the instrument (0.894). This result reinforces the interpretation of P_{it} as a valid and strong instrument: see, e.g., the robustness checks suggested by Angrist and Pischke (2008).

4.2 Evolution of the effect over time

In this section, we investigate the persistence of the policy implementation impact over time. We do so by interacting the treatment coefficient (P) with period (month)-specific time dummy variables for the months following the delivery of the organic-waste bins. The results are presented in Figure 4. As the figure shows, there is a significant decrease in waste production in the first eight months starting from the recording of organic-waste bins ("month zero") in each area. After that period, the effect displays a fuzzy pattern, alternating reductions in waste production with non statistically significant positive coefficients. In other words, the marginal impact of the bin delivery over time is strongest in the first eight months. It is worth noting that such period of time does not coincide with a specific calendar year, given that each area follows a different implementation schedule (see Table 1 for the timing). In other words, the period does not coincide with the payment of a fixed annual fee for the bins, which, on the contrary, may determine a rebound effect.[20] The dynamic of the policy impact is consistent with existing evidence in the context of water and electricity consumption, which shows the impermanence of social norms-based intervention effects after a few months in absence of repeated treatments (Ferraro and Price, 2013; Allcott and Rogers, 2014). The novelty of this result is that, in our setup, the outcome is not directly targeted by the intervention. Thus, a key takeaway from this analysis is that the time pattern of a behavioral spillover resembles the effect of an intervention on a targeted activity. This finding plays an important role also for the economic interpretation of the effects, which we discuss in Section 5.

4.3 Heterogeneous effects: dependency on the point in the outcome distribution

Since we do not observe household-specific characteristics, we cannot estimate the dependency of the effect on pre-treatment characteristics of different subpopulations. Instead, we take an alternative approach and study heterogeneity with respect to the distribution of household waste generation, with the due interpretational caution coming from the fact that it is itself an outcome of the policy, after its implementation.

[20]This remains valid even when accounting for the omission of the months in which bins were delivered but not registered.

We first study the heterogeneity of the effect of the policy on different points of the outcome distribution. Table 6 displays the results of a quantile regression of household monthly waste on enrollment (D1) and information (D2), estimated with household and time fixed effects following Machado et al. (2011). Each column represents a separate regression for the conditional 0.25, 0.5, 0.75, and 0.9 quantiles of household waste (kg). The effect of enrollment is negative for all quantiles and significant for all but the 0.25 quantile. The coefficient for the median quantile (-1.15) is close to the one estimated at the mean in our main regression (-1.05, see Table 3). For larger quantiles (0.75 and 0.9), the coefficients are larger in magnitude than the mean regression coefficient (-1.52 and -1.92). The magnitudes are fairly similar to those of the estimates from our FE and IV-FE specifications. Since we do not have information on household size and composition, we cannot differentiate between the different reasons for this relationship. For example, this pattern could be due to a non-linear decrease of the waste generated by each individual, with households with fewer individuals occupying the lower part of the waste distribution and displaying smaller marginal household waste reductions. Alternatively, households generating more waste may have more capacity for a reduction. The effect can also be a combination of these two possibilities. A lack of household composition data prevents us from getting more direct information on the link between environmental preferences and waste reduction. We address this drawback below with an alternative approach. Table 6 reports also the coefficients for the information brochure. The estimated effect is negative and significant for all quantiles (see columns 1-4), and coefficients are larger in magnitude for the two smaller quantiles (columns 1 and 2).

As a result of the treatment, some households may change rank in the distribution of waste production. This hampers the interpretation of the quantile regression results as an effect on fixed heterogeneous categories of households. To stress this point, we estimate a measure of rank reversal using Wilcoxon's signed-rank test based on pooled pre and post-intervention average measures of monthly waste per household. Table 7 reports the results. We estimate that 64% of the households maintain their quartile rank position, while 19% increase it and 17% shift toward more efficient (lower) quartiles. We can reject the null hypothesis that households maintain their quartile rank at the 10 percent level, with a p-value of 0.073. These results imply that we do not necessarily compare the same households before and after the treatment in the same quantile, even though the measure of rank reversal is relatively low. Rather, a correct way to interpret the quantile regression results is in distributional terms. Our results show that higher quantiles of the distribution of household waste converge toward more efficient (lower) levels of waste production, as a result of enrollment. The impact of the policy is particularly pronounced in the upper tail, and this represents a successful outcome for the intervention from a policy making perspective.

Next, we perform an additional estimation to investigate the relationship between the policy and quantiles of baseline waste production. Baseline waste quantiles may proxy for household characteristics, such as preferences for the environment, efficiency in waste generation, as well as a different household composition and size. We interact the treatment (policy implementation, P) with a categorical variable that allocates households to four quantiles of baseline weight, computed from averages of all monthly collections before the distribution of the brochure. Table 8 reports the results. The policy shows a positive effect in terms of waste reduction only starting from the second quartile. With respect to the coefficient relative to the first baseline quartile (2.2 kg), the interaction between the policy and the following quartiles is always significant and equal to -2, -4.8, and -9.7 kg in the third, fourth, and fifth quintiles, respectively. This increasing interaction mirrors increasing average baseline waste productions across the four groups, showing a higher effect for higher quantiles. This result supports the hypothesis of an increasing scope for improvement in waste reduction along the baseline distribution. It suggests that households that were less efficient before the policy have more room for improvement, which may include not only a reduction in waste generation but also an increase in the disposal of packaging items at recycling stations. Similarly, households in lower baseline quantiles may instead increase their waste due to increased capacity (i.e. an extra bin at no extra fixed cost). An additional possibility is that households that were previously composting privately (which we cannot observe) now adopt the organic-waste bin provided by the municipality, so their organic waste is now registered, mechanically increasing post-intervention waste weight. Nonetheless, a major takeaway from this analysis is that the policy has a significant and larger effect in the least waste-efficient quantiles of the baseline distribution.

4.4 Relation between compliance and the treatment effect

Standard econometric literature views the compliance decision as contingent on potential gains from the treatment, $D_i = \mathbb{1}\{Y(1) \geq Y(0)\}$. In our case, however, Y is not an outcome targeted by D, but an indirect behavioral spillover. We analyze the relation between the decision to participate and the –indirectly targeted – waste generation in two different ways.

Our first (naive) approach is to separately estimate equations (2) and (3) only for the subgroup of compliers. The results are presented in Table 9. The estimates of β_1 for the subsample of compliers (columns 1 and 2) are negative and significant, and of somewhat larger (smaller) magnitude than the corresponding coefficients for the FE (IV FE) estimates with the full sample. On the subsample of noncompliers, this coefficient is not identified. The estimates for β_2 for the subsample of compliers are also negative and significant and very similar to the results for the full sample. Finally, the estimated coefficient of the policy implementation is negative and significant for the compliers.

The results presented in Table 9 are potentially biased as sample splitting according to participation choice corresponds to conditioning on a potentially endogenous variable. We therefore nonparametrically estimate the treatment effect on the group of compliers using a LATE estimator as an additional robustness check. In our case, however, noncompliance is only one-sided: no households enroll in the new organic waste separation option when it is not offered to them. Thus, $\mathbb{E}[D_{1,i}|P_i = 0] = 0$. To account for this, we adjust the LATE estimator in the way suggested by Bloom (1984) (see also Angrist and Pischke, 2008). In particular, ignoring the dependence on time, the average treatment effect on the treated can be expressed as

$$ATE = \frac{\mathbb{E}[Y_i|P_i = 1] - \mathbb{E}[Y_i|P_i = 0]}{\mathbb{E}[D_{1,i} = 1]}. \tag{5}$$

The estimate is equal to -2.931 and the corresponding p-value is 0.020.

We note that a drawback of this approach is that we neglect the panel structure of the data and pool all observations together. The coefficient estimate is higher than in the corresponding panel estimation. A plausible explanation is than panel estimates account for unobservable household characteristics that may correlate with volumes of waste and with the enrollment choice by including household fixed effects, while a pooled regression does not allow to do so. Another possible reason for the higher coefficient is that this estimate does not account for the prior dissemination of information about the benefits of organic waste recycling through a brochure.

5 Interpretation of empirical results

The objective of this section is to discuss an economic interpretation of the negative effects of (i) enrollment in the new system (β_1), (ii) the second brochure (β_2), and (iii) the policy implementation (β_3) on the generated amount of waste in a parsimonious analytical framework. We consider several competing channels and at least two competing economic frameworks.

As for channels, commonly discussed reasons for a change in environmental behavior include a change in the cost or productivity (in a monetary or technical sense) of pro-environmental behavior (Ek and Miliute-Plepiene, 2018), a change in the information set (learning effect) (Allcott and Rogers, 2014), and a change in the attention paid to a certain activity, which is related to information framing (Asensio and Delmas, 2016). A direct distinction between these channels is not straightforward as it is possible that more than one of those factors are at play and induce an effect in similar directions. In this section we aim at pointing out the most likely dominant one.

As a starting point, the monetary channel is unlikely to play a major role in the behavioral spillover. At the time of the second brochure, no actual change to the status quo occurred in terms of price, but we still observe a reduction in waste. In addition, β_2 cannot be plausibly interpreted as a cost-related anticipation effect; such an adjustment is hard to link to any forward-looking rational behavior in terms of organic waste separation because it would still require a special bin. Considering β_1, enrollment in the new bin system actually made it cheaper to generate waste through a lower fixed cost (an organic-waste bin exempt from a yearly fee and a lower annual fee for the residual bin compared with one for unsorted waste), rather than more expensive. In addition, it did not change the price per kilogram of waste. Because of the reduced fixed cost of the residual bin (compared with the one for unsorted waste), there may be a "rebound" effect, i.e., increased domestic waste production in response to a lower price paid.[21] However, the fixed fee is paid annually on the first payment in the new year, with either monthly or quarterly

[21] "Rebound effects consist of an increase in consumption or resource usage due to a decrease in price caused by a targeted intervention and/or technological improvement (Gillingham et al., 2013).

payments; therefore, the fixed fee payment does not generate a systematic incentive for households to "rebound" after eight months. In other words, the rebound should display a different monthly pattern than what we observe; either it should apply to all months or it should be stronger when the fixed fee is paid, but different for each area.

Similarly, the dynamic of the effect over time is hard to explain according to the preference formation or the information set arguments. All of those arguments would predict a long-term change in the behavior, which conflicts with the observed sliding of the behavior back to oscillations around a null effect.

In contrast, our results seem to be compatible with the interpretation of the effect as attention-driven. In particular, waste generation, as a byproduct of consumption, is a repetitive and economically unimportant activity. In addition, the consequences of waste generation are typically not salient to individuals. It is therefore not difficult to imagine households' default behavior as not taking negative externalities into consideration. Both the information treatment and policy enrollment lead to increased attention to waste generation as a spillover from attention to waste separation. Since the attention decreases back to its default over time, the effect vanishes as well. This interpretation is consistent with existing evidence in the context of electricity consumption (Allcott and Rogers, 2014), where households significantly reduce their usage after receiving informative reports but the effect then declines and eventually disappears and backslides in a few months in the absence of repeated treatments.

The interpretation of the behavioral spillover effects as induced by increased salience is straightforward to incorporate into existing behavioral microeconomic models. For an illustration, we use the framework of Gabaix (2014). Suppose that the utility function u of an individual with limited attention is

$$u(a, x, m) = -\frac{1}{2}(a - \sum_{i=1}^{n} b_i(m_i x_i + (1 - m_i)x_i^d))^2, \tag{6}$$

where a is a vector of actions that the individual takes (such as waste reduction, school choices, and consumption of meat), x is the vector containing the true values of each relevant variable, x_i^d is the default value of that variable for the individual, b is a vector of weights, and m is the attention vector with $m = 1$ meaning full attention and $m_i = 0$ no attention paid to variable or activity x_i. When no attention is paid to an activity, its perceived value is equal to the default value assigned by that individual (which is, e.g., formed by a habit). The optimal action taken by an individual given a triple (a, x, m) minimizes the F.O.C. of (6) and is equal to

$$a^* = \sum_{i=1}^{n} b_i(m_i x_i + (1 - m_i)x_i^d). \tag{7}$$

The spillover effects we find can then be interpreted as a correlated shock to the vector m. It is natural to assume that when one activity receives a boost in attention, closely related activities "profit" from that boost. Here, if for example activities 1 and 2 are related, then an increase in m_1 is likely to be associated with an increase in m_2. As a result, a treatment that targets 1 has a spillover effect on action a_2 through the adjustment of the optimal activity a^* as a result of the increased attention to 2.

Here, it is worth discussing the differences between our findings and the mechanism discussed by Ek (2018). The setup considered by Ek (2018) envisions several activities devoted to the same social goal. Individuals allocate fractions of their limited time to those activities. The utility obtained by participating in the production of the social goal is derived though a "self-image function", which relates the contributions to some (exogenously given) ideal amount of contribution (Brekke et al., 2003). In this setup, a policy that increases the productivity of the individual with respect to one of the activities (e.g., provision of two bins for more environmentally friendly waste collection) decreases the amount of effort that the individual spends on other related activities (such as searching for environmentally friendly nondurable goods, in our context). Possible reasons for the prediction of a negative spillover include a time "budget" constraint that links all activities. In addition, an enhanced contribution over one activity may give the agent a *moral license* to behave adversely on other dimensions without increasing the distance to an ideal level of contribution. Our finding of a positive behavioral spillover to household waste seemingly contrasts this hypothesis of moral licensing.

6 Concluding Remarks

Our analysis suggests at least three major policy takeaways. First, the introduction of elective curbside organic waste separation has a high success rate. More than 87% of households adopt the double bin technology or compost privately. Second, curbside organic waste separation produces a positive behavioral spillover in terms of overall household waste reduction, net of recycled packaging items. This implies not only that the municipality collects organic waste that can be used in the production of biofuel, but also that this is accompanied by a reduction in total household waste, with cleaner waste streams. Lastly, our findings exclude that the policy results in a *moral licensing* effect where households feel entitled to generate more waste because they already put effort into separating organic waste.

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7 Tables and Figures

Table 1: Implementation timing by area

	First brochure	Second brochure	Org. waste bin distribution	Official start	Actual recording
Area 1	2 July 2013	6 Sept 2013	Nov 2013	25 Nov 2013	Feb 2014
Area 2	2 July 2013	20 Jan 2014	9 May 2014	19 May 2014	June 2014
Area 3	2 July 2013	4 Aug 2014	Oct 2014	10 Nov 2014	Dec 2014
Area 4	2 July 2013	16 Jan 2015	24 April 2015	11 May 2015	June 2015

Source: authors' elaboration from information obtained by the waste management agency of the Partille municipality.

Table 2: Descriptive Statistics

Panel A: *Household's waste (kg/month, including organic and residual, excl. recycled packaging)*

	N	♯ hh	mean	sd	p50	min	max
Kg/month kg	270,475	4324	30.980	20.725	27.5	0	148.5

Panel B: *Household's waste (kg/month) by sorting choice at policy introduction*

	Residual	**Double bin**	**Unsorted**
N	49204	188656	32812
♯ hh	785	3015	524
Pre-policy mean waste/hh/month (kg)	19.802	34.850	37.236
Std. dev.	13.26	16.453	16.751
After-policy mean waste/hh/month (kg)	16.122	31.834	34.587
Std. dev.	11.785	15.625	15.896

Panel C: *Descriptive statistics by collection area and by sorting choice at policy introduction*

	N	♯ hh	% of hh		**Residual**	**Double bin**	**Unsorted**
Area 1	52337	845	0,20	N	10100	37235	5002
				♯ hh	162	602	81
				%	19.17	71.24	9.59
Area 2	44668	700	0,16	N	8722	33204	2742
				♯ hh	137	520	43
				%	19.57	74.29	6.14
Area 3	46546	729	0,17	N	7679	31607	7260
				♯ hh	120	495	114
				%	16.46	67.90	15.64
Area 4	130736	2050	0,47	N	23346	89101	18289
				hh	366	1398	286
				%	17.85	68.20	13.95

Descriptive statistics from the Partille municipality's administrative waste collection data (2012-2017). Double bin refers to households that adopt an organic-waste and a residual bin. N= number of observations. ♯ hh = number of households.

Table 3: Organic waste separation and household waste production

Outcome: monthly household waste (kg)				
	(1)	(2)	(3)	(4)
VARIABLES	FE	IV2SLS	First stage	FE
Enrollment (D1)	-1.219***	-2.121***		
	(0.249)	(0.203)		
Information (D2)	-1.299***	-0.829***	-0.014***	-0.800***
	(0.219)	(0.183)	(0.001)	(0.184)
Policy (P)			0.894***	-1.896***
			(0.005)	(0.181)
Observations	270,475	270,475	270,475	270,475
R-squared	0.037		0.890	0.037
Number of hh	4,324	4,324	4,324	4,324
Mean of dep. var.	30.98	30.98	0.431	30.98
F-stat first stage (Kleibergen-Paap):			36906.65	

Results from regressing monthly household waste on D1, D2, or P using the full sample from Partille waste collection data. Column 1 reports the fixed effects panel data estimation (FE), column 2 the instrumental variable (IV) estimation results, column 3 the first stage of the IV, and column 4 an FE estimation of the policy impact P and D2. All regressions include household, month, and year fixed effects. Standard errors clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**) or 10(*) percent level.

Table 4: Adoption of organic waste separation and household waste production (**excluding composting households**)

Outcome: monthly household waste (kg) Subsample: excluding household with private compost				
	(1)	(2)	(3)	(4)
VARIABLES	FE	IV2SLS	First stage	FE
Enrollment (D1)	-1.049***	-2.188***		
	(0.280)	(0.239)		
Information (D2)	-0.998***	-0.425**	-0.017***	-0.389*
	(0.249)	(0.211)	(0.002)	(0.211)
Policy (P)			0.870***	-1.902***
			(0.006)	(0.208)
Observations	221,275	221,275	221,275	221,275
R-squared	0.038		0.865	0.038
Number of hh	3,539	3,539	3,539	3,539
Mean of dep. var.	33.85	33.85	0.420	33.85
F-stat first stage			2988.873	

Results from regressing monthly household waste on D1, D2, or P using a subsample of households from Partille waste collection data which excludes those with a private composting device. Column 1 reports the fixed effects panel data estimation (FE), column 2 the instrumental variable (IV) estimation results, column 3 the first stage of the IV, and column 4 an FE estimation of the policy impact P and D2. All regressions include household, month, and year fixed effects. Standard errors clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**) or 10(*) percent level.

Table 5: Placebo test: lagged policy implementation and household waste production

Outcome: monthly household waste (kg)				
	(1)	(2)	(3)	(4)
VARIABLES	FE	IV2SLS	First stage	FE
Placebo enrollment D1	0.303	0.345		
	(0.200)	(0.213)		
Placebo policy P			0.915***	0.316
			(0.005)	(0.195)
Observations	76,696	76,692	76,696	76,696
R-squared	0.051	0.051	0.916	0.051
Number of hh	4,276	4,272	4,276	4,276
Mean of dep. var.	32.59	32.59	0.395	32.59

Results from regressing monthly household waste on placebo representations of the organic waste separation policy (D1 and P), using the full sample of households from Partille waste collection data. We exclude from the sample all observations that follow the reception of the first information brochure (i.e. July 2013). The policy implementation timing is lagged back in time and replicated by maintaining the same distance between the four areas (for example, placebo treatment P is February 2012 for Area 1). Column 1 reports the fixed effects panel data estimation (FE), column 2 the instrumental variable (IV) estimation results, column 3 the first stage of the IV, and column 4 a FE estimation of the policy impact P. All regressions include household, month, and year fixed effects. Standard errors clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**) or 10(*) percent level.

Table 6: Quantile regression, fixed effects estimation

Outcome: monthly household waste (kg)				
	(1)	(2)	(3)	(4)
Quantiles:	0.25	0.5	0.75	0.9
Enrollment (D1)	-0.85	-1.15***	-1.52***	-1.92***
	(0.58)	(0.39)	(0.27)	(0.42)
Information (D2)	-1.38**	-1.31***	-1.23***	-1.14**
	(0.68)	(0.45)	(0.31)	(0.49)
Mean of dep. var.	30.98	30.98	30.98	30.98
StDev of dep. var.	20.73	20.73	20.73	20.73
N	270,475.00	270,475.00	270,475.00	270,475.00

Results of quantile treatment effects obtained from a quantile regression of monthly household waste on D1 and D2, using the full sample from Partille waste collection data. Each column reports the coefficient corresponding to a specific quantile (the 25th, 50th, 75th, and 90th percentiles in columns 1, 2, 3, and 4, respectively). All regressions include household, month, and year fixed effects. Standard errors clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**) or 10(*) percent level.

Table 7: Wilcoxon signed-rank test

Sign	N (hh)	%
Positive	825	19.159
Negative	740	17.185
Zero	2741	63.655
All	4306	100
Ho: qt0 = qt1		
z = 1.790		
Prob $>\mid z \mid$ = 0.0734		

The table displays the results of a Wilcoxon signed-rank test of a rank change in the relative distribution of household monthly waste quartiles. The measure of rank reversal is based on pre- and post-intervention (brochure distribution) average measures of monthly waste per household. The null hypothesis corresponds to the absence of a change in quartile before and after distribution of the first information brochure.

Table 8: Organic waste separation policy and household waste (kg/month). Heterogeneous effects by quartiles of baseline waste production

Outcome: monthly household waste (kg)	
	(1)
	FE All
Policy introduction P	2.227***
	(0.289)
Policy * 2nd quartile	-2.017***
	(0.382)
Policy * 3rd quartile	-4.818***
	(0.389)
Policy * 4th quartile	-9.720***
	(0.441)
Brochure	-0.787***
	(0.181)
Observations	270,245
Number of hh	4,318
R-squared	0.055
Mean of dep. var.	30.97

Results from a fixed effects panel data regression of monthly household waste on the brochure D2 and the policy P, which is interacted with four indicators of the households' respective quartiles of baseline (pre-policy) average monthly waste weight. We use the full sample of households from Partille waste collection data. The regression includes household, month, and year fixed effects. Standard errors clustered at household level. Asterisks denote statistical significance at the 1(***), 5(**) or 10(*) percent level.

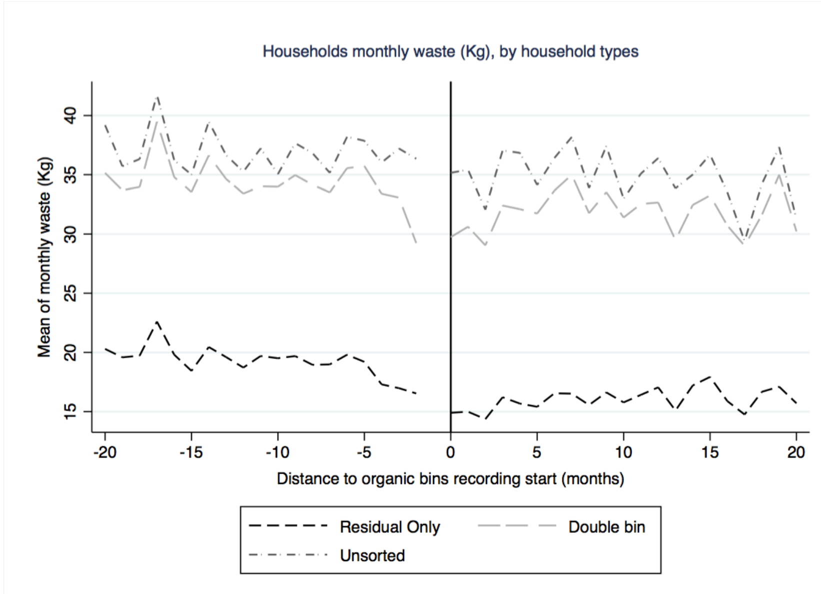
Table 9: Enrollment in organic waste separation and household waste production (**compliers**)

Outcome: monthly household waste (kg) Subsample of compliers (composting and double-bin households)				
	(1) FE	(2) IV-2SLS	(3) First stage	(4) FE
Enrollment (D1)	-2.068***	-2.009***		
	(0.197)	(0.191)		
Information (D2)	-0.982***	-1.018***	0.001	-1.019***
	(0.195)	(0.192)	(0.000)	(0.192)
Policy (P)			1.000***	-2.009***
			(0.000)	(0.191)
Observations	237,734	237,734	237,734	237,734
R-squared	0.039		0.998	0.039
Number of hh	3,800	3,800	3,800	3,800
Mean of dep. var.	30.27	30.27	0.482	30.27
F-stat first stage (Kleibergen-Paap):			3711.718	

Results from regressing monthly household waste on D1, D2, or P using the subsample of complying households from Partille waste collection data (i.e., those with a private composting device or who adopt an organic-waste bin). Column 1 reports the fixed effects panel data estimation (FE), column 2 the instrumental variable (IV) estimation results, column 3 the first stage of the IV, and column 4 an FE estimation of the policy impact P and D2. All regressions include household, month, and year fixed effects. Standard errors clustered at household level. Standard errors are clustered at the household level. Asterisks denote statistical significance at the 1(***), 5(**) or 10(*) percent level.

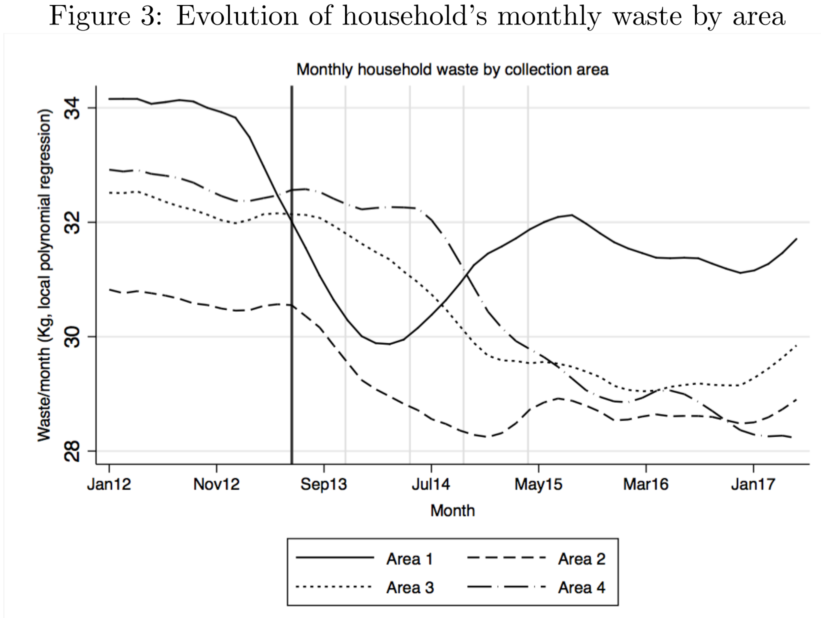
Figure 1: The four areas of staggered organic-waste collection implementation in the municipality of Partille (Sweden). Source: Partille Municipality

Figure 2: Evolution of household monthly waste by household type (regime of collection)



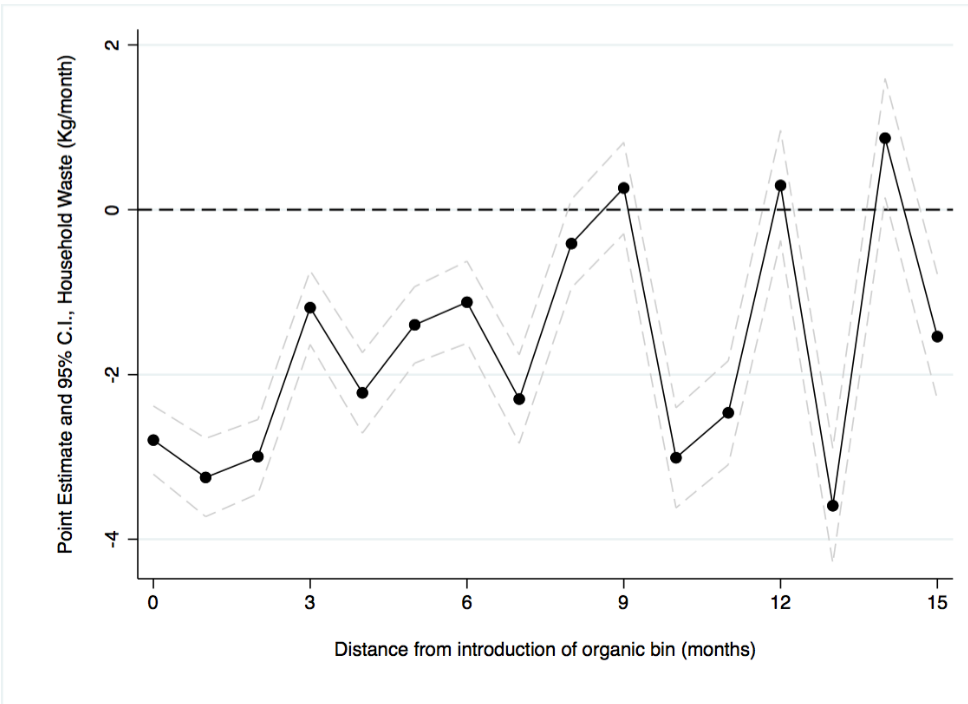
Authors estimation from Partille administrative waste collection data. N=271,311, with 4,342 unique households. The vertical black line corresponds to the policy introduction (organic-waste bin recording). The two previous months are omitted due to a systematic omission of organic waste weight in the municipal waste agency recordings.

Figure 3: Evolution of household's monthly waste by area



Local polynomial regression, bandwidth 5. The sample includes all monthly observations from January 2012 to April 2017. The black vertical line corresponds to the first brochure mail-out. Grey vertical lines indicate the months in which organic-waste bins were distributed in each area (1 to 4). The huge drop for area 1 is due to the fact that organic-waste bins were distributed but the waste management company started recording their weight only three months later and the smoothing function amplifies the effect. Figure A.1 in the Appendix displays mean weights per month by area.

Figure 4: Effect of the policy introduction on household monthly waste over time

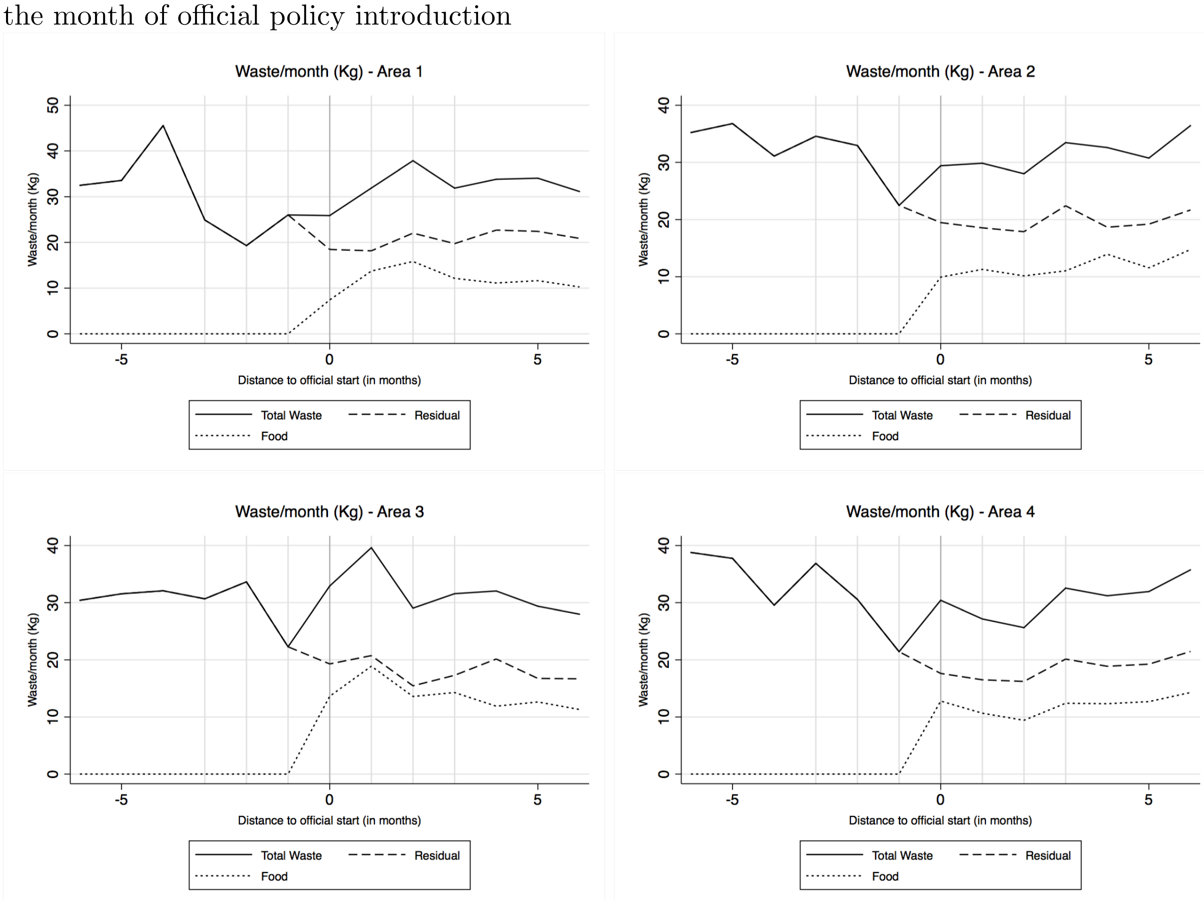


The figure reports the coefficient estimates and 95% confidence intervals obtained from the interaction between the policy P and a month-specific dummy variable for each period after the bins collection, from the full sample of households in the Partille administrative waste collection data. Month zero corresponds to the first collection of organic waste bins. Months 15 and above are pooled. The dependent variable is household monthly waste in kg, net of recycled packaging items. The regression includes household, month, and year fixed effects and the brochure dummy variable. Standard errors clustered at household level.

Appendix

Figure A.1 below reports the mean weight of households monthly waste separately for the four policy implementation areas. We can observe that, one month prior to the actual recording (vertical line), the amounts of waste per household registered by the collection company miss a component and we can attribute it to organic waste. The figure highlights how the amounts of residual and total waste coincide, while organic waste was not being reported. By looking at the difference between residual and total waste in the following months, we can observe that the one observed in month “-1 corresponds approximately to the non-reported organic waste.

Figure A.1: Evolution of organic and residual waste for households adopting the two bins, around the month of official policy introduction



Source: Authors estimation from Partille administrative waste collection data. The sample is restricted to households that adopt the organic-waste bin at time 0, for each area. The figure shows that some misreporting occurred in month 0 (which corresponds to the “official policy start but not to the start of the actual recording) for all areas (and months +1 and +2 for Area 1): total household waste coincides with the residual waste, revealing that organic waste was not recorded.